induced tumor growth and metastatic spread through antiangiogenic and immunostimulatory mechanisms.

Accumulating clinical evidence suggests that β-AR antagonist, and propranolol in particular, may increase survival and decrease recurrence in patients with breast cancer.[3] Barron et al[2] have studied the associations between use of β_2-AR (propranolol) or β_1-selective antagonists (atenolol) and risk of breast cancer. They show that propranolol is associated with significantly less advanced disease at diagnosis and lowers breast cancer-specific mortality. Furthermore, Pasquier et al[3] strongly suggest that propranolol with chemotherapy may improve the outcome of women with breast cancer. In contrast, a recent study found no protective effect of propranolol (which represented 16% of the β-blockers prescribed) and increased recurrence rates associated with metoprolol.[4]

Propranolol is also a new and emerging treatment for melanoma, multiple myeloma, pancreatic cancer, head and neck squamous cell carcinoma cell lines, or severe infantile haemangioma.[5] It would thus be interesting to evaluate the risk of breast cancer according to the subtype of β-blockers and, in particular, nonselective β-blockers. Would it be possible for the authors of this article to provide breast cancer risk data according to the type of β-blockers?

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In Reply We appreciate the comments of Hugon-Rodin et al and Ji and Chen and respond to the issues they raised. Hugon-Rodin et al raised the question of whether the relationship between β-blocker use and breast cancer risk varied when results were stratified according to type of β-blocker. As reported in our article,[1] overall we found no relationship between current use or long-term current use (for ≥10 years) of β-blockers and risk of invasive ductal breast cancer (odds ratio [OR], 0.9 [95% CI, 0.7-1.2], and OR, 1.1 [95% CI, 0.7-1.8], respectively). No appreciable variations in risk were seen when we analyzed risks according to current use of β_1-selective blockers vs nonselective β-blockers. Specifically, current users of β_1-selective blockers of any duration and for 10 years or more had ORs of 0.9 (95% CI, 0.7-1.2) and 1.2 (95% CI, 0.7-1.9), respectively, and current users of nonselective β-blockers of any duration and for 10 years or more had ORs of 0.8 (95% CI, 0.4-1.6) and 0.8 (95% CI, 0.3-2.4), respectively. However, 90% of control women who were current β-blocker users were users of a β_1-selective blocker (the other 10% were current users of nonselective β-blockers), limiting our power to detect differences in risk between β_1-selective blockers vs nonselective β-blockers.

Ji and Chen asked about the influence other potential confounders may have had on our risk estimates, specifically heart disease and duration of hypertension history. Adjustment for heart disease did not appreciably influence the magnitude or direction of any of the risk estimates reported in Table 2 of our article. Specifically, in Table 2, the risk estimate associated with current use of calcium channel blockers for 10 years or more (OR, 2.4 [95% CI, 1.2-4.9]) did not change after additional adjustment for history of heart disease (OR, 2.4 [95% CI, 1.2-5.0]). With respect to duration of hypertension history, we defined this as years between age at initial hypertension diagnosis and reference age. This variable is highly correlated with duration of antihypertensive use, and thus we only assessed its potential to confound current antihypertensive use risk estimates. None of our current use risk estimates reported in Table 2 were statistically significant, and additional adjustment for duration of hypertension history did not appreciably change the magnitude or direction of any of these estimates. We did not collect information on hypertension severity (how well hypertension was controlled), and our ascertainment of only self-reported dose information limited our ability to evaluate dose-response relationships.

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Treatment of Osteoporotic Vertebral Fractures

To the Editor McCullough et al[1] reported that apparent mortality reductions following vertebroplasty and kyphoplasty likely result from selection bias. These findings are important to counter hasty conclusions from several recent population-based studies and reviews. Yet these new results are hardly surprising, given the a priori lack of evidence to assume a causal association between vertebral fractures and mortality, let alone mortality risk modifiable by targeting the spine. Reduced pulmonary function following vertebral fracture would be a plausible mechanistic explanation, but only small, short-term improvements in vital capacity following vertebral augmentation have been dem-

onstrated. Contrary to the frequently cited increased risk of pulmonary deaths in older women with prevalent vertebral fractures in the Study of Osteoporotic Fractures, incident vertebral fractures (arguably the stronger outcome) in that cohort were no longer associated with mortality following adjustment for other determinants such as frailty.[2] The same authors have further shown that hyperkyphosis and height loss are equally unfavorable, independent of vertebral fractures and their characteristics,[3] arguing against a direct role of the latter.

On the other hand, the underlying osteoporosis, as well as frailty and disability resulting from fractures, are known major direct causes of subsequent mortality, especially if the next fracture occurs at the hip. Moreover, not only population-based studies but also several recent randomized clinical trials and meta-analyses suggest that while tremendous underrecognition and undertreatment of osteoporosis persists, treatment of high-risk individuals probably does have indirect mortality benefits.[4] Thus, although it was omitted from the discussion of McCullough et al[1] and the accompanying Invited Commentary,[5] it cannot be overemphasized that spinal augmentation procedures are no substitute for appropriate evaluation and treatment of underlying fracture risk.

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To the Editor We read with interest the article by McCullough et al.[1] The authors have used the Medicare data set to perform a mortality analysis and cited our study[2] as being in contrast to their findings. Their study suggests that, because of selection bias, our finding that vertebral augmentation had a measurable and positive effect on mortality was incorrect. However, we stand by our finding and note several important limitations of the study by McCullough et al.[1]

- Their preprocedure subgroup (augmentation >30 days after vertebral compression fracture [VCF]) analysis was flawed in excluding most augmentation patients (71.3%). Essentially they failed to consider the majority of augmentation patients, who may have needed emergent care, which could explain why their treatment was within 30 days of VCF.
- McCullough et al[1] stated that the augmentation group was healthier (hence the selection bias), but their own data shows a significantly *lower* Quan comorbidity score and lower rates of prior inpatient admissions as well as chronic pulmonary disease for the control group, which is strongly suggestive of improved health state in the *control group*.
- They did not stratify the treatment group by treatment type. Because 71% of their patients underwent vertebroplasty, their treatment cohort would be biased.
- They only considered baseline comorbidities, ie, they failed to account for conditions that led to the VCF. Furthermore they considered only a limited set of comorbidities compared with other similar studies that included more comorbidities.
- They failed to consider that their analysis included health care utilization and major medical complications prior to the augmentation for the treatment group, ie, unrelated to the procedure. This approach favors the control group.

Thus, while the data and results in the article by McCullough et al[1] may be accurate as stated, they do not accurately represent or estimate the overall outcomes following VCF in the entire Medicare population.

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1. McCullough BJ, Comstock BA, Deyo RA, Kreuter W, Jarvik JG. Major medical outcomes with spinal augmentation vs conservative therapy. *JAMA Intern Med*. 2013;173(16):1514-1521.

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In Reply We appreciate the comments of Edidin and colleagues as they touch on key elements of our study,[1] and their previous article[2] provides insightful contrast to our own.

We included the "preprocedure subgroup" analysis to illustrate that a substantial proportion of the augmented group (29%) had a markedly lower risk of complications compared with controls despite being "theoretically" equivalent—both groups had the same treatment during this time (no augmentation), and we controlled for baseline characteristics, including Quan comorbidity scores, prior inpatient admissions, and chronic pulmonary disease, among others, using traditional multivariate models. Edidin et al are right to be concerned that these traditional multivariate models might not adequately account for acute differences in health at the time, such as patients needing emergent care. We agree. There are many other clinical details available in real-time

that are not evident in billing claims data. The entire clinical picture at presentation, past and present, will influence therapeutic decisions as well as eventual patient outcomes. We suggest selection bias is the unmeasured factor allowing 2 "theoretically" equivalent groups to have such different outcomes.

We did consider the possibility that clinicians may preferentially perform spinal augmentation on the sickest patients so as to maximize their chances of survival—emergent spinal augmentation, if you will. Complications should then be highest immediately before the procedure, as patients clinically declare their need for more aggressive care. As Figure 2B in our article illustrates, however, preprocedure complications were less than postprocedure complications.[1] In fact, there was a nadir in complications immediately before the procedure and a spike immediately after the procedure. We interpret these results to indicate that the majority of patients are not undergoing augmentation because of dire clinical need.

Several methodological clarifications may be helpful. First, we did not stratify by treatment type (vertebroplasty vs kyphoplasty) in our analysis. However, we have compared these 2 treatments using these data and found no benefit of kyphoplasty over vertebroplasty. A recent randomized clinical trial comparing vertebroplasty with kyphoplasty also did not find benefit of one over the other.[3] Second, we collected "baseline" comorbid conditions over the entire year prior to fracture. We included osteoporosis in that list, presumably the cause of the fracture. Unlike Edidin et al,[2] we excluded all patients with diagnoses of cancer so as not to mix fractures due to metastases with those due to osteoporosis. Third, we used the date of fracture as baseline so that both groups would be compared relative to the same exposure (fracture) rather than starting the comparison when the augmented group had their procedure, which could be as late as 6 months later.

Finally, Edidin et al failed to note in their letter that at the time of their article demonstrating the mortality benefits of spinal augmentation and, in particular, kyphoplasty, over conservative therapy, Dr Edidin was an employee and stockowner of Medtronic Inc, a manufacturer of kyphoplasty supplies.

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Actual Involvement vs Preference for Involvement as an Indicator of Shared Decision Making

To the Editor The article by Tak and colleagues[1] about the association between patient preferences for shared decision making and hospital resource use touches on a very timely topic. Many Western health care systems are preparing for the challenges of aging patients who are increasingly coping with the chronic consequences of disease rather than acute disease itself.

Central in this transition is to (re)establish a patient-physician partnership because in the many situations where "preference-sensitive" decisions[2] are to be made, unwarranted practice variation is large.[3] Shared decision making is expected to result in more satisfied patients who receive care better aligned with their preferences and in lower health care spending.[4]

The study by Tak et al[1] adds to the evidence base data showing that hospital patients' preferences for being involved in medical decision making is related to higher rather than lower costs, an observation seemingly opposed to the aforementioned hypothesis—or not? The authors related patient *preferences* for their involvement in hospital resource use, not their actual involvement. Although it is likely that patients who want to be involved are more involved, the two are not the same. This observation is crucial for the interpretation of these results: for individuals, patient preference for involvement and their actual involvement may in fact be reversely related, where previous experiences of too little involvement may increase preferences for being involved but—given unchanged physicians' behaviors—at the same time still result in low levels of actual involvement. In addition, the relations between patient preference, actual involvement, and resource use may be confounded by such factors as disease severity, coping styles, or hopes for the future—factors difficult to adjust for. Finally, this study was performed in an acute care hospital setting, whereas a primary care setting may have been more appropriate to evaluate this research question. This may also be reflected in the fact that more than 70% of the patients interviewed preferred "to leave medical decision making to their physicians."

Shared decision making can only be a successful strategy for cost containment and increased patient satisfaction, if patients and health care professionals mutually agree on shared decision making as the basis of health care provision and continuously apply it in their ongoing communication about the best-fitting care.

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